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Interactive comment on "Impact of meteorological clouds on satellite detection and retrieval of volcanic ash during the Eyjafjallajökull 2010 and Grímsvötn 2011 eruptions: a modelling study" *by* A. Kylling et al.

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Response to interactive comments from Referee #4

We thank the referee for the careful reading of and constructive comments to our manuscript. The referee's comments are repeated below in italic font. Our responses to the comments are shown in roman font.

Specific comments

1. *The title of the manuscript concerns (specifically) the effects of meteorological clouds on satellite retrievals, yet large parts of the Abstract, Discussion and Conclusions sections are actually focused on the under-detection of ash and the underestimate of retrieved ash mass loading compared to the "Flexpart" data, which are not directly related to the presence or not of clouds. I would suggest considering amending the title of the manuscript to reflect this extended focus, or else reducing the focus of the paper so as to concentrate more strongly on the cloud-free/cloudy aspects.*

   We have reduced the focus of the paper and not included the underestimate of retrieved ash mass loading compared to the "Flexpart" data in the Abstract and the Conclusions. The last part of the title has been changed from "A modelling study" to "A modelling sensitivity study" to highlight that this is a sensitivity study.

2. *The Abstract is very long, and I feel is far too detailed for a scientific abstract. Please consider whether this could be shortened significantly.*

   The Abstract has been shortened significantly. See also answer to previous question.

3. *Page 11306, line 11: It's not very clear what is meant by "experimental methods" in this context, and lines 10-14 are generally unclear. Please try to clarify this text.*

   This part has been re-written.

4. *Section 2: Other than references to Kylling et al. (2013), there seems to be little or no reference to other previous work on simulated satellite imagery in the presence of volcanic ash.*

   A reference to the paper by Millington et al. (2012) has been added.

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5. *Page 11308, lines 8-10. I was worried that there might be a slightly "incestuous" element to the Eyja analysis, in that the Flexpart data for the Eyja cases have already incorporated the effects of SEVIRI data via the data inverse modelling, and these Flexpart data are then directly compared with simulated and real SEVIRI data. Is the whole process entirely self-consistent?*

   As stated in the Introduction the aim is to investigate the effect of ice and liquid water clouds on detection and retrieval of volcanic ash. This is done by comparing simulated SEVIRI-like images with and without ice and liquid water clouds. The Flexpart derived ash fields are indeed based on SEVIRI measurements. However, only quantitiave comparisons are made between simulated and measured images, as stated in the introduction. All findings presented are based on model-model comparisons. We have added additional text to the second to last paragraph in the Introduction to clarify this. Furthermore we have replaced the measured SEVIRI brightness temperatures differences in Figs.âĹij3 and 4 by similar simulated cloud SEVIRI images. The measured SEVIRI brightness temperatures differences have been included in Fig. 7.

6. *Page 11309, lines 12-14: I was worried about this treatment of water vapour as a constant profile, and felt it should be justified. Subsequently, it does get justified in the Discussions section. Perhaps there could be better "sign-posting" to anticipate this (i.e. referring forward to the Discussions)?*

   We have added a sentence referring forward to the Discussions, as suggested.

7. *Page 11309: There is no mention of which surface emissivity data are used for the radiative transfer calculations. Given the subsequent discussions on detection efficiency as a function of cloudiness (which must be strongly related to spectral surface properties in some way?), I think this is important.*

   The source of the surface emissivity data have been added.

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8. *Page 11309, lines 20-22: I don't understand "...standard deviation of the simulated brightness temperature was 0.25 K for more than 94% of the pixels...". What (physically or mathematically) is the standard deviation in simulated BT for a single pixel?*

   This has been clarified in the revised manuscript.

9. *Page 11310, lines 18-23: Firstly, you use the phrase "uniform (mono-disperse)" surely these two things contradict each other? Doesn't a uniform size distribution imply a uniform distribution of differently-sized particles (e.g. same numbers at each size in a number distribution), whereas a mono-disperse distribution implies that all particles have the same size? But secondly, I don't think that the use of equation (1) does imply a mono-disperse distribution, I think it is applicable to size distributions of finite width, with Qext then becoming the mean extinction efficiency for that distribution (rather than the extinction efficiency for a sphere of radius r_e, which is how I read your equation). In any case, r_e doesn't really have any meaning for a mono-disperse size distribution – since all particles have the same radius, the effective radius reduces to exactly that radius!*

   The wording was wrong here. It was meant to state that the size distribution does not vary within the pixel. This has been clarfied in the revised document.

10. *Page 11311, line 10: By stating that the state vector consists of only the 10.8 micron optical depth and the effective radius, you're effectively saying that every other variable is known, including the surface and ash cloud temperatures. The ash cloud temperature chosen will have a profound influence on the subsequently retrieved mass loadings, yet the method you use (taking the coldest 12.0 micron BT from a 29 x 29 pixel box) is surely liable to significant error? Can you comment? (Is this the coldest ash pixel, or the coldest from all pixels?) Do you use 29 x 29 boxes for both real and simulated satellite images (given their differing spatial resolutions)?*

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As stated in the Introduction, assumptions have to be made about ash composition, type and size distribution. Furthermore, surface and ash cloud temperatures have to be estimated either from the images directly or from weather forecast information. We choose to use the information in the images. We have not performed an analysis of the mass loading uncertainty due to the uncertainty in the ash cloud temperature. It is noted that the use of weather forecast models will also have a similar uncertainty. The best approach would be to have independent altitude information about the ash cloud altitude from for example CALIPSO or one could use the altitude information from the dispersion model.

The coldest pixels within the box is used. For simulated images a 10 x 10 pixel box is used. This information was left out from the manuscript and has been added.

11. *Page 11311, line 13: I don't think the value of (10)^2 for the optical depth error variance could have come from the Francis et al. (2012) paper, since the Francis scheme doesn't use optical depth as a state variable.*

    This have been corrected in the revised manuscript.

12. *Page 11312, lines 25-27: I don't understand where "These differences are attributed to uncertainties in representation of cloud and temperature fields and the coarser spatial resolution in the simulations" comes from. Please clarify exactly what you mean.*

    The sentence has been clarified.

13. *Page 11313, line 26: You use the phrase "good agreement" here - good in what sense? Spatial consistency? The mass loading values themselves seem quite different between, for example, top-left in Fig 1 and left-hand in Fig 5.*

    We have re-phrased this sentence and it now reads: “The retrieved ash mass loadings based on the cloudless simulated images (left plots) show the same

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maxima and minima structures as the the Flexpart ash distributions (Figs. 1 and 2), but are smaller in magnitude, see discussion in section 5 for an explanation."

14. *Page 11313: At the bottom of this page, you say that "...including meteorological clouds causes both over- and under-estimates of the ash mass loading compared to the cloudless situation...", but I don't see any discussion of what mechanisms might cause this effect (unless they're elsewhere in the paper and I seem to have missed them)?*

    This is discussed at the end of section 4.1. We have included a reference to this section in the revised manuscript.

15. *Top of page 11314, lines 1-4: Are you comparing detection here, or mass loadings? Figs 5, 6 and 7 show loadings, but then you refer to Fig 3, which is detection only. So when you say "better represent the measurements", do you mean for detection or loading?*

    It should read Fig. 5 on line 6. This has been corrected.

16. *Page 11315, lines 3-4: Where you say that "Clouds have a variable impact on the number of pixels identified as ash (compare solid and dashed green lines)", you might also say that this also acts in the same sense (on average), in that the dashed green line always lies above the solid green line.*

    Change made as suggested.

17. *Page 11315, line 10: Is there a typo here? You say that "There appears to be NO strong dependence in the ash detection on the satellite viewing angle as demonstrated by the green lines in Fig. 9", and then proceed to demonstrate (to my eyes) that there is a dependence. Or am I missing something?*

    The lack of satellite view dependence apply to coincident pixels. This information has been added.

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18. *Page 11315, lines 16-27: You talk about the effects of low-level inversions here, but surely, to affect the detection sensitivity, then there has to be a spectral effect, since it is the 10.8-12.0 difference which needs to change to affect the ash detection? Since water vapour profile is constant, doesn't this have to be a surface emissivity effect? I think this should be addressed in the text for clarification.*

    In the simulations the surface emissivity does not change between day and night. For the cloudless simulations the only changes (other than the changes in the ash field) are changes in the temperature fields. Prata and Grant ("Retrieval of microphysical and morphological properties of volcanic ash plumes from satellite data: Application to Mt Ruapehu, New Zealand", Q. J. R. Meteorol. Soc., 127, 2153-2179, 2001) in their Eq. 5, have shown that the 10.8-12.0 temperature difference changes when the surface temperature changes with everything else held constant. We have added the Prata and Grant (2001) reference to the text.

19. *Page 11316, lines 8-9: What is the significance of "These are associated with increased emissions of ash on 15 May (Stohl et al., 2011)"? This doesn't explain why these were not detected. Is it just because the mass loadings were too low?*

    The mass loadings were large enough to be detected, but the pixels were missed due to the presence of clouds. This explanation has been added to the text.

20. *Page 11316, lines 13-18: You argue that it's the low ash altitude that causes this reduced detection. What is your evidence for asserting this? Couldn't it also be that the mass loadings are too small?*

    This section was not clearly phrased. As stated in lines 6-7, too small mass loadings may also cause the reduction. The paragraph has been rephrased accordingly.

21. *Page 11316, lines 21-23: Does the "mean of the number of pixels detected as ash relative to Flexpart ash pixels for each scene in the cloudy simulations" include false positives? i.e. is it (green + red)/blue, or just green/blue?*

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The numbers do not include false positives. This information has been added to the text.

22. *Page 11317, lines 1-2: There seems to be a self-contradiction here, because you refer to 8 May as a case where "more pixels are identified as ash for the cloudy than for the cloudless simulation" on the one hand, and then refer to 6-8 May as a contrasting case on the other - i.e. 8 May is common to both!*

    It is correct that on 8 May more pixels are identified as ash for the cloudy than for the cloudless simulation. However, as is evident from Fig. 8 the difference is small. We have rewritten the sentence in the revised manuscript to clarify this.

23. *Page 11317, lines 17-18: When you say "the brightness temperature difference will be smaller for the cloudy scene", which BTD are you referring to? Therefore, I don't quite follow the reasoning behind the statement that "Both these factors interact to cause both over- and underestimates of the ash mass loading" - please clarify.*

    The ΔT depends on the temperature of the radiatively effective surface below the ash cloud, whether that is the Earth's surface or a cloud layer. A cloud will typically have a lower temperature than the Earth's surface and hence ΔT will be smaller for an ash cloud above an underlying cloud than if that cloud was not present. The text has been clarified in the revised manuscript.

24. *Page 11317, lines 20-26: It's somewhat misleading (to me at least) to include the "also false positives" phrase, since the addition of just false positives would tend to decrease the under-estimate, rather than increase it (as is the case as presented). You are obviously adding both the false positives and the false negatives, and I think the text should be clarified accordingly. The same argument applies to the Grimsvotn case on page 11319, line 25.*

    We realise that these statements may cause confusion and have left them out in the revised manuscript. See also response to comment 1.

25. *Page 11319, lines 14-16: Are these pixels missed because the mass loading is too small, then?*

    For the pixels between 10-12 km, clouds are not the reason for them not being detected, rather the mass loading is too small. This has been clarifed in the text.

26. *Page 11319, line 18 and Figures 13 and 16: You say "April", which should clearly be "May".*

    It should be May. Corrected. Thank you.

27. *Page 11320, lines 12-13: You talk about "the presence [of detected ash] in the cloudless simulated scenes, lower right plot Fig. 12" – as hard as I tried, I couldn't see any ash pixels over Scandinavia in this plot!*

    Scandinavia is wrong, it should read "North Sea". This has been corrected.

28. *Page 11321, line 13: The sentence "For coincident pixels both over- and under-estimations of the mass loading happens" seems particularly redundant since it follows on from the phrase "reveals both under- and overestimates of the mass loading due to the presence of clouds within a single scene" which has almost directly preceded it! But actually, the whole of this paragraph (lines 13-20) seems superfluous, since it merely restates what has already been presented in the paper, and adds no further discussion.*

    The paragraph has been removed.

Technical comments

1. *Page 11306, line 8: I suggest inserting a comma into "To do so, cases with volcanic ash..."*

    Change made as suggested.

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2. *Page 11310, line 14: Missing "t" in "They required that aT least 6 out of 9 pixels..."*

   Correction made.

3. *Page 11319, line 8: Suggest "false positive pixels" rather than "false positives pixels".*

   Change made as suggested.

4. *Page 11323, lines 21-22: Spelling of "underestimateed".*

   Correction made.

5. *Page 11324, lines 7-8: "...to get a as complete as possible picture..." needs to be rephrased.*

   The sentence has been rephrased.

6. *Figure 5 caption: What is the sign of the difference? i.e. is it cloudless minus cloudy, or vice versa? Presumably the differences are only (can only?) be plotted for coincident pixels?*

   The difference is cloud minus cloudless. This information has been added to the caption. We have also added that the difference is for pixels identified as ash in both the cloudy and cloudless simulations.

7. *Figure 8: Why bother with a reference to a different right y axis – it seems to be identical with the left-hand axis?*

   The reference to the right y axis has been removed.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 11303, 2014.